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Interactive comment on "A field validated surrogate model for optimum performance of irrigated crops in regions with shallow salty groundwater" *by* Zhongyi Liu et al.

Anonymous Referee #1

Received and published: 17 February 2020

The manuscript presents a field validated surrogate model to simulated various hydro-agricultural variables in an irrigation field with shallow salty groundwater.

The subject is relevant, and the analysis is interesting, however, the manuscript has a sequence of structural flaws which needs to be addressed. In particular:

1. The title mentions the optimum performance of irrigated crop. Optimization is however NOT a topic covered by the analysis, and optimum crop performances are neither reached nor explored. I agree that the simulation model can support irrigation management, and I suggest to re-phrase the title accordingly.

2. Overall, the authors present too much information about the important role of irriga-

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tion, and too little and confused regarding the tradeoff between irrigation and salinity.

3. More information on the current status of surrogate modelling in shallow aquifers is needed since it is not clear how the proposed approach contributes with respect to the current status.

4. The methodology is quite clear and thorough, even though it can be lighter if some textbook material is simplified and properly referred to.

5. It would be interesting to present, at the beginning of the methodology, a methodological framework which includes all the experimental steps and summarizes the field and modelling effort, highlighting the interdependences between the two components.

6. The results could be structured differently (some simulation results appear to be presented beforehand)

Minor comments: L59: Add this information in a separate sentence, providing context on the total extension of the basin.

L 97: I recognize that the objective here is to introduce the need for more surrogate models for irrigation areas with shallow aquifers. However, this sentence appears not connected with what stated before.

L 98-104: I believe the flow of thoughts here should be: 1- There are limited modelling resources when GW is near the surface. 2- Shallow aquifers areas are in fact different from their physical. characterization perspective (i.e. explain better lines 94-104). 3- If any modelling has been performed, it is necessary to provide some context (what did Xue et al., 2018; Gao et al., 2017; Liu et al., 2019 do? what were the shortcomings of their modelling experience?). How the current manuscript contributes towards implementing a more reliable-simple-tailored model in the specific application?

L189: Not clear. Do you mean: j is the exogenous variable on which the term before the parenthesis depends?

L 339: Groundwater?

L 466: I would specify that the SA used in this experiment is a qualitative one

L467: outputs?

L472: I wonder if experimental data should be presented in the case-study characterization, and not in the result section.

L473: calibration and validation results

L595: There is no red line

L626: However, information on calibrated and simulated trajectories of those variables are already shown (see for example fig 7). I would re-name the current section or (even better), restructure the results to complement the above discussion with error statistics.

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